Dr. D. G. Brinton then presented his views of the subject.

The three original speakers were then called upon and each supplemented his remarks by thoughts suggested by the others.

In the course of his remarks, Prof. Cope exhibited two specimens illustrative of generalized types of Vertebrata. One of these was a cast of a species of the genus Phenacodus, from the Eocene, which represents the family from which all the Ungulate Placental Mammalia have descended. The other was a part of the skeleton of a reptile from the Permian, of the new genus Otocœlus. This genus is the type of a new family of the order Cotylosauria. This order approaches most nearly of all the Reptilia to the class Batrachia. It is also the most generalized of the Reptilia, and from it all other orders of the class have probably descended by modifications in different directions. The particular family Otocœlidæ differs from the other families of Cotylosauria in the possession of a meatus auditorius externus and of an osseous carapace. From it were probably descended the orders of Pseudosuchia and Testudinata, which first appear in the Trias. A description of this family and the species it includes will be given in an early number of the PROCEEDINGS of the Society.

Nominations 1346 to 1362 were read.

The Society was then adjourned.

The meeting of the first of May having been designated as that on which a discussion of the theme, "Factors of Organic Evolution," should be held, Prof. Cope, to whom the Special Committee in charge of the preparations for this meeting had confided the task of opening the discourse, presented an epitome of the subject as it exists to-day from the standpoint of paleontology.*

* [Prof. Cope, being unwilling to furnish the Society with the text of his remarks, or to have the stenographic copy printed in the PROCEEDINGS, his part of the joint discussion must be necessarily omitted.—SECRETARIES.]

Discussion of the Factors of Organic Evolution from the Embryological Standpoint.

By Prof. E. G. Conklin.

(*Read before the American Philosophical Society, May 1, 1896.*)

Up to the beginning of this decade embryology was largely dominated by the phylogeny idea. Individual development was generally studied, as the paleontologist studies his fossils, with a view to deciphering the evolutionary record in the various stages. It is now generally recognized, however, that embryology is but little fitted for the service into which it was so long forced, viz., the determining of phylogenies. The only safe guide in this matter is comparative anatomy of both living and extinct forms. On the other hand, our knowledge of the mechanics of evolution must always depend in large part upon the study of individual development. More than any other discipline, embryology holds the keys to the *method of evolution.* If ontogeny is not a true recapitulation it is, at least, a true *type* of evolution, and the study of the causes of development will go far to determine the factors of phylogeny.

The causes and methods of evolution are intimately bound up with those general phenomena of life such as assimilation, growth, differentiation, metabolism, inheritance, and variation; and the evolution problem can never be solved except through a study of these general phenomena of life itself. Our great need at present is not to know more of the course of evolution, but to discover, if possible, the causes of growth, differentiation, repetition, and variation. All these general phenomena are most beautifully illustrated in the development of individual organisms, and because they are fundamental to *any* theory of evolution I shall dwell upon them rather than upon the evidences for the Lamarckian or the Darwinian factors.

I call your attention very briefly to the following propositions: 1. Development, and consequently evolution, is the result of the interaction of extrinsic and intrinsic causes. 2. Intrinsic causes are dependent upon protoplasmic structure. 3. Inherited characters must be predetermined in the structure of the germinal protoplasm. 4. Germinal, as compared with somatic, protoplasm is relatively stable and continuous, but not absolutely so as maintained by Weismann; therefore, extrinsic causes may modify both germinal and somatic protoplasm. 5. It is extremely difficult to determine whether or not extrinsic factors have modified the structure of the germinal protoplasm. This is illustrated by some of the evidences advanced for the inherited effects of (1) diminished nutrition, (2) changes in environment, (3) use and disuse. 6. Experiment alone can furnish the crucial test of these Lamarckian factors.

1. The causes of development in general are usually recognized as twofold, extrinsic and intrinsic. As examples of extrinsic causes may be mentioned gravity, surface tension, light, heat, moisture, and chemism in general; examples of intrinsic causes are the non-exosmosis of salts from living bodies in water, the pouring of a glandular secretion or the sap of plants into a cavity under high pressure, the active changes in shape and position on the part of cells, assimilation, growth, division, etc. There is not, however, a uniformly sharp and distinct line of demarcation between these two factors of development. Phenomena once supposed to be due entirely to intrinsic causes are now known to be the result of extrinsic ones, and it is practically certain that this will be found true of still other phenomena. But although it is not possible to draw any hard and fast line between these two classes of causes, one can, in general, recognize a very marked difference between them. Extrinsic causes may, in large part, supply the stimulus and the energy for development, and may more or less modify its course; the intrinsic causes are of a much more complex character than the extrinsic ones, they are inherent in the living matter and in large part predetermine the course of development. In one form or another the distinction between these two classes of causes is recognized by all naturalists. His calls the intrinsic causes "the law of growth," the extrinsic ones the conditions under which that law operates. These designations correspond, at least in part, to Prof. Cope's Anagenesis and Katagenesis, and to Roux's "simple and complex components" of developmental processes.

While it is necessary to emphasize the differences between these two classes of causes, it is not intended thereby to dogmatically assert their total difference in kind. It may well be that these extrinsic and intrinsic causes are totally different in kind, but in our present state of ignorance it would be unjustifiable to affirm it. On the other hand, it would be just as unwarrantable to dogmatically affirm that there is no difference in kind between these two classes of causes, and that, therefore, all vital phenomena are only the manifestations of heat, light, electricity, attraction, repulsion, chemism, and the like. It may be that this is true, but there is as yet no sufficient evidence for it, and to attempt, as certain dynamical and mechanical hypotheses do, to refer all vital phenomena directly to such simple components as those named above is practically to make impossible at present any explanation of vital phenomena. "If we would advance without interruption," says Roux,* "we must be content, for many years to come, with an analysis into complex components."

2. We need not now further concern ourselves with an explanation of *extrinsic* causes or *simple* components, since this subject properly belongs to chemistry and physics. If, however, we examine more closely some of the *intrinsic* causes or *complex* components, we will find

* Wilhelm Roux, *Einleitung. Archiv für Entwicklungsmechanik der Organismen.*

that they are always associated with more or less complex ***structures; in fact, that they are dependent upon structure.***

The smallest and simplest mass of protoplasm that can manifest all the fundamental phenomena of life, such as assimilation, growth, division, and metabolism, is an entire cell, nucleus and cytoplasm, and probably centrosome. The cell is composed, as microscopic study plainly reveals, of many dissimilar but perfectly coadapted parts, each performing its specific function, and it may therefore properly be called an *organism.* Some phenomena of cell life may be directly referred to the various visible constituents of the cell, but many of them are evidently connected with structures which we cannot see, structures which may perhaps never be seen, and yet which must be vastly more complex than the most complex molecules known to chemistry, and yet much more simple than the microsomes, centrosomes, and chromosomes which are visible in the cell. With these ultra-microscopical particles many of the most fundamental phenomena of life are associated, viz., assimilation, growth, metabolism, and probably differentiation, repetition, and variation. These functions are so coördinated that there can be no question that the ultra-microscopical structure is an *organization,* with part coadapted to part. The organization of the cell, therefore, does not stop with what the microscope reveals, but must be supposed to extend to the smallest ultimate particles of living matter which manifest specific functions. These are the vital units so generally postulated, the "smallest parts" of living matter, as they were called by Brücke, who first demonstrated that they must exist; the "physiological units" of Spencer, the "gemmules" of Darwin, the "micella-groups" of Nageli, the "pangenes" of De Vries, the "plasomes" of Wiesner, the "idioblasts" of Hertwig, the "biophores" of Weismann. Such ultimate units have been found absolutely necessary to explain those most fundamental of all vital phenomena, *assimilation* and *growth,* while many other phenomena, especially *particulate inheritance,* the *independent variability of parts,* and the hereditary transmission of *latent* and *patent characters,* can at present only be explained by referring them to ultra-microscopical units of structure. To deny that there are such units does not simplify the problem, as some seem to suppose, but renders it impossible of approach. A corpuscular hypothesis of life, like that of light, may be only a temporary makeshift, but it is better than nothing.

Whitman* well says: "Brücke's great merit consists in this that he taught us the necessity of assuming structure as the basis of vital phenomena, in spite of the negative testimony of our imperfect microscopes. That function presupposes structure is now an accepted axiom, and we need only extend Brücke's method of reasoning, from the tissue cell to the egg cell, in order to see that there is no escape from the

* C. O. Whitman, *The Inadequacy of the Cell-Theory of Development,* Biological Lectures, 1893.

conclusion that the whole course of developmental phenomena must be referred to organization of some sort. Development, no less than other vital phenomena, is a function of organization."

3. A study of the phenomena of development, as well as the principle of causality, make it certain that all the characters of the species are predetermined within the protoplasm of the fertilized egg cell. From a frog's egg only a frog will develop, from an echinoderm egg only an echinoderm, and the course of the development is, under normal circumstances, definitely marked out in each case, even down to the minutest details. All the results of experiment, as well as observation and induction, only serve to render this conclusion the more certain. It should be observed that to affirm that characters are predetermined is a very different thing from saying they are preformed. The one merely asserts that the cause of the transformations which lead from one step to another in the development is determined by the initial conditions of the fertilized egg cell; the other affirms that those transformations have already taken place.

The absolute determinism of development depends primarily upon the constant structure of the egg cell, but also to a certain extent upon a definite relation to extrinsic factors. Since, however, these extrinsic factors may be exactly the same in two cases, and yet the result of development be very different (*e. g.*, the egg of the starfish and that of the sea urchin), we can only conclude that while ontogenetic differences may be caused by a disturbance of the extrinsic factors, *inherited characters* are always the result of a definite structure of the germinal protoplasm, and that, therefore, development is, in the words of Prof. Whitman, "a function of organization."

Inheritance and variation are general terms which include a great many different kinds of phenomena, many of which seem to be due to entirely different factors. A great many phenomena of inheritance seem to be due entirely to extrinsic forces, but a more careful inquiry always reveals the fact that they are invariably due to the reaction of certain extrinsic causes on a perfectly definite living structure. As examples may be mentioned the following:

(1) The tiger-like striping of the egg of Fundulus, which is very characteristic and would certainly be regarded as an inherited character, has been shown by Loeb* to be due entirely to the position of the blood vessels of the blastoderm. The pigment cells are at first uniformly distributed, but when the blood vessels are formed they gather around them, probably through chemotropic action, and thus the characteristic banded appearance is produced. Graf has since shown that the color paterns of leaches are produced in the same way. It is not necessary, therefore, to assume that the color paterns in these cases are specifically represented in the germinal protoplasm; it may

* Jacques Loeb, *Some Facts and Principles of Physiological Morphology*, Biological Lectures, 1893.

even be that the position of the blood vessels is not so represented, but there must be some ultimate cause back in the germinal plasm itself which determines the series of causes which finally produces the color paterns. In short, this feature, like most others, was predetermined from the beginning.

(2) Herbst * has shown in a series of interesting experiments that by the use of various chemical substances the development of echinoderms may be profoundly modified. For example, in sea water deficient in calcium-chloride, or in which there is an excess of potassium-chloride, the pluteus larva, instead of developing calcareous spicules and the long ciliated arms which give the normal larva an angular, easel-shaped appearance, remains rounded in shape much like the larva of Balanoglossus, in which no spicular skeleton is developed. The withdrawal, therefore, of certain normally present substances from the environment may profoundly modify the end result. But in this case, as in the other, it is absolutely certain that the calcareous spicules were predetermined in the egg cell, although in the absence of calcareous matter from the water those spicules could not be built—the plan was there, but the building material was lacking.

Such modifications resulting from unusual conditions of pressure, temperature, density, nutrition—in fact, any alteration of the chemical or physical environment—may appear in any stage of development from the unsegmented egg to the adult condition, but it must not be supposed that the entire development can be reduced to such factors. Loeb argues that we do not inherit our body heat from our parents because it depends upon certain chemical processes, but is it not absolutely certain that we inherit a certain protoplasmic structure which determines those chemical processes, and hence the body temperature? To assume that extrinsic causes determine whether there shall hatch from an egg a chicken or an eagle is the sheerest nonsense. The study of extrinsic factors in relation to inheritance will serve to simplify some of the intricate problems to be explained, but surely no one believes that development can ever be referred entirely to such factors. The fact is that determinism, which is the most fundamental characteristic of inheritance, is manifested at every step of development, and there is certainly no escape from the conclusion that this determinism depends upon protoplasmic structure, and that this structure it is which is transmitted from generation to generation and which forms the physical basis of inheritance.

All really inherited characters must, therefore, be represented in the structure of the germinal protoplasm, and must consequently be present from the beginning of development. "We must consider it as a law derivable from the causality principle," says Hatschek, † "that in

* *Zeit. wiss. Zool.*, Bd. lv.

† Berthold Hatschek, *Ueber die Entwicklungsgeschichte von Teredo*, Arb. Zool. Inst., Wien, 1880.

the phylogenetic alterations of an animal form the end stages are not alone altered, but the entire series from the egg cell to the end stage. Every alteration of an end stage or addition of a new one must be caused by an alteration of the egg cell itself." Nägeli * has expressed a similar view in the following famous sentence: "Egg cells must contain all the essential characteristics of the species as perfectly as do adult organisms, and hence they must differ from one another, no less as egg cells than in the fully developed state. The species is contained in the egg of the hen as completely as in the hen, and the hen's egg differs as much from the frog's egg as the hen from the frog."

4. The remarkable tenacity of inheritance, as shown especially in reversions and the preservation of useless and embryonic characters through many hundreds or thousands of generations, and amid the most diverse circumstances, bears strong testimony to the great stability of that living structure which is the basis of inheritance. On the other hand, all experience goes to prove that the living substance of the body cells in general is readily modified, and that in a surprisingly short time. The fact of this great difference cannot fail to be recognized; its cause is at present merely a matter of conjecture.

Weismann at one time supposed the cause of this to be an absolutely stable, absolutely separate, and perpetually continuous germ plasm. However, there is the most convincing and abundant evidence that although the germ plasm is relatively very stable and continuous, it does not possess those divinely perfect characters ascribed to it. More recently Weismann has expressly abandoned each and all of these characters,† and now, like a good Lamarckian, finds "the cause of hereditary variation in the direct effects of external influences on the biophores and determinants."

The outcome of the whole matter, then, is that we find ourselves much in the same position as we were before Weismann denied the possibility of the inheritance of acquired characters. *All hereditary variations are caused by the action of extrinsic forces on the germinal protoplasm, producing changes in its structure.* Strangely enough, this proposition was admitted as a logical necessity by one who undertook by rigorous logic to prove the reverse. Since almost the only objection to this position was the one raised by Weismann, it may now be considered as definitely settled, and the only question before us, then, is: How can extrinsic causes modify the structure of the germinal protoplasm?

Since by his own admissions, as Romanes has shown, the most characteristic features of Weismann's system, both as to inheritance and evolution, have been virtually abandoned, it seems to some that his theories have been of no real value, and that, like an *ignis fatuus*, they have only served to lead biologists astray far from the path of science into the dangerous quagmires of speculation. I do not share any such

* Nägeli, *Mechanisch-physiologische Theorie der Abstammungslehre*, 1884.

† See Romanes' *Examination of Weismannism*, 1893.

opinion. Apart from his splendid observations and the great stimulus to investigation which Weismann's theories have furnished, there remain many elements of permanent value in his work.

Osborn * thinks that Weismann's most "permanent service to biology is his demand for direct evidence of the Lamarckian principle." It seems to me that his greatest service consists in the emphasis which he has laid upon the intrinsic factors of development and evolution as opposed to the extrinsic factors, a thing which he has indeed overemphasized, but which has sadly needed a strong defender in these later years. Largely as an outcome of his work, we now recognize the possibilities and the limitations of the selection theory as never before, and we also recognize that many of the evidences which were adduced in support of the Lamarckian factors are not conclusive, while the method of securing conclusive evidence is clearly marked out. Whatever we may think of his theories, this certainly is no slight service.

5. It is by no means an easy task to determine whether the influence of extrinsic forces has really reached the germinal protoplasm and modified its structure; much more difficult is it to determine how that modification takes place. I believe it is safe to say that a majority of the cases which are supposed to prove the inheritance of acquired characters prove only that characters are acquired, not that they are inherited. There is great need of caution against supposing that any character is inherited unless it repeats itself under many and different conditions. Apart altogether from inheritance, similar conditions may produce similar results, and consequently this source of error must be eliminated if we would be certain that the structure of the germinal protoplasm has really been modified. Many of the alleged cases of the inheritance of mutilations, of the direct influence of the environment and of use and disuse fall away under this precaution.

The general evidence for the inheritance of mutilations is so notoriously bad that I pass it by altogether, and select for consideration a few cases, chosen from a recent work on the subject,† which have by various writers been alleged as showing the direct influence of environment in modifying species and also the inherited effects of use and disuse.

(1) It is well known that certain gasteropods, if reared in small vessels, are smaller than when grown in large ones, and this case has been cited as showing the influence of environment in modifying species. There is good evidence, however, that this modification does not affect the germinal protoplasm, for these same gasteropods will grow larger if placed in larger vessels. It seems very probable that the diminished size of these animals is due to deficient food supply, but this has so little modified the somatic protoplasm that, although they may be fully developed as shown by sexual maturity, they at once

* Osborn, *The Unknown Factors of Evolution*, Biological Lectures, 1894.

† E. D. Cope, *The Primary Factors of Organic Evolution*, 1896.

increase in size as soon as more abundant food is provided, and this takes place by the active growth and division of all the cells of the body. In higher animals, once maturity has been reached, there is little chance for growth, apparently because many of the cells are so highly differentiated that they can no longer divide. Consequently the growth is limited, and hence the size of the adult may depend in part upon the amount of nutriment furnished to the embryo. This limitation of growth is due to the high degree of differentiation of the somatic cells. But as the germ cells are not highly differentiated and are capable of division, it follows that they would not be permanently modified by starving. It may be, as Prof. Brewer argues, that long continued starving and consequent dwarfing of animals may leave its mark on the germinal plasm; but, as he also remarks, this influence must be very slight as compared with the cumulative effects of selection in breeding, and it is safe to assert that there is no such wholesale and immediate modification of the germinal plasm due to the influence of nutrition as some people seem to suppose.

(2) The interesting experiments of Schmankewitsch in transforming one species of Artemia into another by gradually increasing the salinity of the water, or in transforming Artemia into another genus, Branchinecta, by decreasing the salinity of the water are well known, and are often cited as illustrations of the fact that specific and even generic differences may suddenly be produced under the influence of the environment. The very fact, however, that these changes are suddenly produced, and that they can at will be quickly modified in one direction or the other is evidence that they are not represented in the structure of the germinal plasm, and the fact that definite extrinsic causes, such as salt or fresh water, acting upon this plasm produces results which are constantly the same is the best evidence that the internal mechanism, *i. e.*, the structure of the germinal plasm, is constantly the same. The same can be said of many artificially produced modifications, such as the exogastrulas and potassium larvæ of Herbst, all of which profound changes are due entirely to extrinsic and not to intrinsic causes, as is shown by the fact that they disappear as soon as the immediate extrinsic cause is withdrawn. The same thing is shown in Poulton's experiments on the colors of Lepidopterous larvæ, and in this case also it is known that the changes are not inherited, at least during the limited period through which the experiments were conducted; and it should be observed that to assume that this would take place at the end of an indefinite number of generations is simply to beg the question.

Very many other cases of a similar character might be instanced under this head if time permitted, but I hasten on to another class of evidence.

Under the subject of the inherited effects of use and disuse the following cases may be mentioned as showing how inconclusive much of this evidence is:

(1) In the first place, this whole line of argument starts with the assumption that the individual habits of an animal are inherited, and that these habits ultimately determine the structure—an assumption which really begs the whole question ; for, after all, the substratum of any habit must be some physical structure, and if modified habits are inherited it must be because some modified structure is inherited. I take an example which will serve as an illustration of a whole class : Jackson* says that the elongated siphon of Mya, the long-necked clam, is due to its habit of burrowing in the mud, or to quote his words : "It seems very evident that the long siphon of this genus was brought about by the effort to reach the surface, induced by the habit of deep burial." It certainly would be pertinent to inquire where it got this habit, and how it happened to be transmitted. It is surely as difficult to explain the acquisition and inheritance of habits, the basis of which we do not know, as it is to explain the acquisition and inheritance of structures which are tangible and visible. Such a method of procedure, in addition to begging the whole question, commits the further sin of reasoning from the relatively unknown to the relatively known !

This case is but a fair sample of a whole class, among which may be mentioned the following : The derivation of the long hind legs of jumping animals, the long fore legs of climbing animals, and the elongation of all the legs of running animals through the influence of an inherited habit. All such cases are open to the very serious objection mentioned above.

(2) Another whole class of arguments may be reduced to this proposition : Because necessary mechanical conditions are never violated by organisms, therefore modifications due to such conditions show the inheritance of acquired characters. Plainly, the alternative proposition is this : If acquired characters are not inherited, organisms ought to do impossible things.

(3) Many of the arguments advanced to prove the inheritance of characters acquired through use or disuse seem to me to prove entirely too much. For example, Prof. Cope argues very ably that bones are lengthened by both stretch and impact, and that modifications thus produced are inherited. Even granting that this is true, how would it be possible for this process of lengthening to cease, since in active animals the stretch and impact must be continual ? Prof. Cope answers that the growth ceases when "equilibrium" is reached. I confess I cannot understand this explanation, since the assumed stimulus to growth must be continual. But granting again that growth may stop when an animal's legs become long enough to "satisfy its needs," how on this principle are we to account for the *shortening* of legs, as, for example, in the turnspit dog and the ancon sheep and numberless cases occurring in nature ? If any one species was able, by taking thought of mechanical stresses and strains, to add one cubit unto its stature, how could the same stresses and strains be invoked to decrease its stature ?

* R. T. Jackson, *Memoirs Boston Soc. Nat. Hist.*, 1890.

These evidences are, I know, not the strongest ones which can be adduced in support of the Lamarckian factors. There are at present a relatively small number of such arguments which seem to be valid and the great force of which I fully admit. But the cases which I have cited are, I believe, fair samples of the majority of the evidences so far presented, and in the face of such "evidence" it is not surprising that one who is himself a profound student of the subject and a convinced Lamarckian prays that the Lamarckian theory may be delivered from its friends.*

6. Another line of evidence, and by far the most promising, is that of direct experiment. So far most of the experiments which have been carried on to determine this question have been carried only half way to a conclusion—they have shown that characters are acquired, they have usually failed to show that they are transmitted to descendants. Among animals one of the best known cases is the inheritance of epilepsy and other disorders in Guinea pigs, due to certain nervous lesions of the parents. But Romanes,† who spent much time in trying to corroborate these results, concludes as follows: "On the whole, then, as regards Brown-Sequard's experiments, it will be seen that I have not been able to furnish any approach to a full corroboration."

Among plants, on the other hand, there is more and better experimental evidence, but it is not by any means as full or satisfactory as could be wished. Of one thing we may be certain : a satisfactory solution of the problem can be reached only by experiment. The mere observations and inductions of the morphologist, while affording valuable collateral evidence, can never furnish the crucial test. As long as we deal merely with probabilities of a low order there will be profound differences of opinion : *e. g.*, Cope believes in all the Lamarckian factors; Romanes rejects use and disuse, but believes in the others; Weismann rejects all of them. Why? Is it because each does not know the facts upon which the others build? Certainly not. Those so-called facts are merely probabilities of a higher or lower order, and to one man they seem more important than to another. No conviction based even upon a high degree of probability can ever be reached in this way. There is here a deadlock of opinion, each challenging the other to produce indubitable proof. This can never be furnished by observation alone. Possibly even experiment may fail in it, but at least it is the only hope.

Conclusion.

On the whole, then, I believe the facts which are at present at our disposal justify a return to the position of Darwin. Neither Weismannism nor Lamarckism alone can explain the causes of evolution. But Darwinism can explain those causes. Darwin endeavored to show that

* H. F. Osborn, *Evolution and Heredity*, Biological Lectures, 1890.

† G. J. Romane , *Post-Darwinian Questions*, 1895.

variations, perhaps even adaptations, were the result of extrinsic factors acting upon the organism, and that these variations or adaptations were increased and improved by natural selection. This is, I believe, the only ground which is at present tenable, and it is but another testimony to the greatness of that man of men, that, after exploring for a score of years all the ins and outs of pure selection and pure adaptation, men are now coming back to the position outlined and unswervingly maintained by him.

Finally, we ought not to suppose that we have already reached a satisfactory solution of the evolution problem, or are, indeed, near such a solution. "We must not conceal from ourselves the fact," says Roux, "that the causal investigation of organism is one of the most difficult, if not the most difficult, problem which the human intellect has attempted to solve, and that this investigation, like every causal science, can never reach completeness, since every new cause ascertained only gives rise to fresh questions concerning the cause of this cause."

The Factors of Organic Evolution from a Botanical Standpoint.

By Prof. L. H. Bailey.

(Read before the American Philosophical Society, May 1, 1896.)

THE SURVIVAL OF THE UNLIKE.

We all agree that there has been and is evolution; but we probably all disagree as to the exact agencies and forces which have been and are responsible for it. The subject of the agencies and vehicles of evolution has been gone over repeatedly and carefully for the animal creation, but there is comparatively little similar research and speculation for the plant creation. This deficiency upon the plant side is my excuse for calling your attention, in a popular way, to a few suggestions respecting the continuing creation of the vegetable world, and to a somewhat discursive consideration of a number of illustrations of the methods of advance of plant types.

1. *Nature of the Divergence of the Plant and Animal.*

It is self-evident that the development of life upon our planet has taken place along two divergent lines. These lines originated at a common point. This common life-plasma was probably at first more animal-like than plant-like. The stage in which this life-plasma first began to assume plant-like functions is closely and possibly exactly preserved to us in that great class of organisms which are known as mycetozoa when studied by zoölogists and as myxomycetes when studied by botanists. At one stage